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SECTION OF GEOLOGY AND MINERALOGY

NOVEMBER 7, 1938

PROFESSOR CHESTER R. LONGWELL, Yale University: *The Basin Range Problem.* (This lecture was illustrated by lantern slides.)

The term *Basin Range System* was first proposed by Gilbert, to designate the "orographic province which has its type in the Great Basin, but is not coincident with it."[1] He recognized that mountain ranges of one general type, structurally and physiographically, occupy the vast area of interior drainage between the Wasatch Mountains and the Sierra Nevada, and additional large areas with interior drainage extending far to the south and southeast. Gilbert was the first also to pose the fundamental problem presented by this system of ranges, when he stated that "the movements of the strata by which ridges have been produced have been in chief part vertical along planes of fracture, and have not involved great horizontal compression."[2] This concept, new in geologic thought, appeared to contradict the views of Clarence King who, in his report of the surveys along the fortieth parallel (1867–1877),

[1] Gilbert, G. K., *Report upon geographical and geological explorations and surveys west of the one-hundredth meridian*, vol. 3, p. 22, 1875.

[2] *Idem*, p. 42.

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sought to explain the ranges of central Nevada and western Utah as greatly eroded anticlines.

Powell suggested a history of the Basin Ranges in which both King's and Gilbert's concepts find a logical place. Folding of the region was succeeded by a long interval during which the surface was nearly levelled by erosion. Block-faulting then fashioned the ranges and basins in their present form, except for modification by later erosion and deposition.[3] Essentially this view has been widely accepted. However, fault-block mountains were not recognized as an important class in other parts of the world for many years after Powell included the "Basin Range Type" in his systematic classification of mountains. As late as 1896, LeConte commented that the Basin Ranges might be the only representatives of their class.[4]

W. M. Davis found the Basin Ranges a fascinating subject, and wrote several papers in which he supported the views of Gilbert and Powell.[5] To assign proper credit for recognition of the major stages in Basin Range development, Davis referred to the "King mountains," formed by folding in mid-Mesozoic time; the "Powell surface," developed during the late Mesozoic and early Tertiary; and the "Gilbert blocks," representing large-scale block-faulting in late Cenozoic time. Later, on learning that important compressive deformation affected formations as young as mid-Tertiary, Davis commented that discovery of these younger structural features does not invalidate his general scheme, but merely adds one or more generations of fold mountains to the stage preceding development of the "Powell surface."

With continued field investigation, the known history of the Basin and Range province grows ever more complex. Three major episodes of compressive deformation are now recog-

[3] Powell, J. W., *Report on the geology of the eastern portion of the Uinta Mts. and a region adjacent thereto*, pp. 32–34, 1876.

[4] LeConte, J., *Elements of geology*, 4th edition, p. 277, 1896.

[5] See, among others, *The mountain ranges of the Great Basin*, Bull. Mus. Comp. Zool., Geol. Ser., vol. 6, no. 3, pp. 129–177, 1903; *The Basin Range problem*, Proc. Nat. Acad. Sci., vol. 11, no. 7, pp. 387–392, 1925; *Basin Range types*, Science, vol. 76, pp. 241–245, 1932.

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nized: The first, which occurred near the end of the Jurassic period, is recorded in close folds and large thrusts in the ranges of western Nevada; the second, dated as "Laramide," caused gigantic thrust faults in the eastern part of the province; the third, post-Miocene in date, formed equally impressive thrust faults in a wide belt running through eastern California and western Nevada. Block faulting on a large scale began before some of the great thrusts developed, and has been continuous or recurrent in parts of the region until recent time. Thus the three major stages suggested by Davis are not clearly separable, unless thrust faulting entered into the formation of the "Gilbert blocks."

The Basin Range problem is both geomorphic and structural. Gilbert and Davis were concerned primarily with the origin of the ranges as surface forms; and their major point—that most of the ranges owe their existence to movements on large faults—is becoming more firmly established as field evidence accumulates. The structural problem consists in determining the kinds and age relations of the structural features, and construction of a plausible synthesis relating these features to regional forces. Since a large part of the region is still to be mapped geologically, a geometrical picture of the province can be constructed only by extrapolation from scattered sample areas—an extremely dangerous practise in a district of complicated geology. Field work now in progress in the Death Valley area is bringing to light major structural features hitherto unsuspected. Any attempted synthesis based on the incomplete data now in hand must be highly speculative. Some of the major questions and suggestions that may be ventured at present are as follows:

1. Late Tertiary structural features of the region speak eloquently of compressive as well as tensional stresses. Has there been alternate application of these two types of stress to the entire region, or have tensional stresses developed locally during regional compression?

2. Some of the major tectonic lines are strikingly parallel

to the great active shear zones in California. Were the Basin Range faults developed in connection with movements along northwesterly shear zones?

3. During the Tertiary there was colossal volcanism in the Basin Range province and adjacent regions. Was this igneous activity merely a response to the large-scale faulting, or did it reflect subcrustal conditions that favored the crustal break-up?

LaConte's simple concept of a collapsed arch hundreds of miles wide now has few if any adherents. However, the "intumescent lavas" of his hypothesis may have been an important factor in weakening the crust, especially in the Great Basin. The systematic pattern made by the ranges in the entire province suggests a yielding to tectonic stresses that were similar in a remarkably wide region. However, it does not seem possible to explain the latest thrusts, the steep "Basin Range" faults, and the tilting of range blocks as results of the same regional forces.

SECTION OF BIOLOGY

NOVEMBER 14, 1938

DOCTOR WENDELL M. STANLEY, Rockefeller Institute for Medical Research, Princeton, N. J.: *The Nature of Viruses.* (This lecture was illustrated by lantern slides.)

In 1892 Iwanowski discovered that the juice from plants diseased with tobacco mosaic remained infectious after being passed through a Chamberland filter, a device that was considered to hold back all known bacteria or living organisms. Despite the fact that he was unable to demonstrate the presence of bacteria in the infectious filtrate, he concluded that the disease was bacterial in nature. Six years later Beijerinck repeated and extended the filtration experiments and concluded that the disease was not caused by ordinary bacteria but by a "contagium vivum fluidum." Thus, although Iwanowski was the first to demonstrate the existence of one of the group of infectious disease-producing agents that we now call viruses, Beijerinck was the first to recognize the true significance of the results and the fact that viruses differ from ordinary bacteria. Although the property of filterability was the earliest recognized property of viruses and has continued to be considered generally as characteristic of viruses, it should be noted that it has not been found an infallible means of distinguishing between viruses and bacteria. Today viruses are recognized not by the sole means of their filterability but by means of a set of general properties that emphasize not only their small size and the fact that they may change or mutate, but especially the intimate relationship that exists between viruses and their host cells. This relationship is manifested by the fact that viruses grow or multiply only within the cells of certain hosts and have never been found to grow on cell-free media, the fact that many virus-infected cells contain inclusion bodies, and the fact that a host recovered from a virus disease is usually, but not always, immune from a second attack of the

same or related virus. Since some of the properties that are used to characterize viruses, such as their reproduction, mutation, and specificity of action, have been regarded for years as being characteristic of living organisms, there has been a general tendency to regard viruses as submicroscopic living organisms somewhat similar to the bacteria.

Early in 1935 there was isolated from mosaic-diseased Turkish tobacco plants an unusual high molecular weight nucleoprotein possessing the properties of tobacco mosaic virus. The same protein was isolated from many different batches of diseased Turkish tobacco plants or from other mosaic-diseased plants such as tomato, common nightshade, petunia, spinach, or phlox. It was impossible to demonstrate the presence of material other than virus protein in purified preparations even by the sensitive precipitin or anaphylactic reactions. The protein was found to be highly active as a precipitinogen, but only weakly anaphylactogenic when tested *in vivo* and inactive when tested *in vitro* by the Schultz-Dale technique. The protein was found to be homogeneous in electrochemical sense and with respect to sedimentation constant. The ultraviolet absorption spectrum was found to agree essentially with the destruction spectrum of virus activity. Denaturation of virus protein preparations by any of several procedures always resulted in a corresponding loss or decrease in activity. At hydrogen ion concentrations more acid than about pH 2 or more alkaline than about pH 8, the protein was split into components of lower molecular weight with loss of virus activity. It was found impossible to separate the virus activity from the protein by any one of several different procedures, such as by filtration through collodion or other types of filters or by centrifugation of the protein from solution under a variety of conditions. Treatment of the virus protein with formaldehyde caused loss of activity, a decrease in amino nitrogen, and a decrease in the color developed by Folin's tyrosine reagent. It was found possible to reactivate the virus to a considerable extent and to demonstrate that the

reactivation was accompanied by an increase in amino nitrogen and in the color developed with the tyrosine reagent. The molecules of the protein are regarded as being rod-shaped because concentrated solutions are liquid crystalline and because dilute solutions show positive stream and electrical double refraction and the Ganz depolarization effect. The results of ultra-filtration and X-ray diffraction studies and various combinations of viscosity, sedimentation, stream double refraction, and diffusion data indicate that the molecules have a molecular weight of at least 43 millions, a length of at least 430 mμ, and a diameter of about 12 mμ. The English workers regard the purified virus protein to consist of aggregates of smaller units present in untreated preparations and have stressed the fact that on purification their preparations lost activity and the ability to pass membranes of average pore size, 450 mμ, and gave an increased amount of stream double refraction. However, it has been found in the Institute laboratories that the protein isolated by careful ultra-centrifugation is essentially the same as the protein as it exists in the plant juice. The virus activity, sedimentation constant, filterability, and stream double refraction of virus protein in juice and following purification were found to be the same. The results indicate not only that the virus protein can be isolated in an unchanged condition but that the protein isolated is tobacco mosaic virus.

Since the isolation of tobacco mosaic virus, over a dozen different specific and highly characteristic macromolecular proteins possessing the properties of the respective viruses or virus strains have been isolated. Although these materials have not been studied as extensively as tobacco mosaic virus, there is no reason at the present time to believe that they differ fundamentally. It becomes necessary, therefore, to consider virus activity in terms of these proteins. However, certain difficulties arise, for the virus proteins have the properties of molecules, whereas virus activity, involving reproduction, mutation and specificity of action, has been generally regarded

as a property of living organisms. At present at least two theories have been proposed to explain virus activity and this apparent discrepancy. According to one, the cell thory of life would be discarded and the virus proteins accepted as a special type of elementary living organisms, whereas, according to the other, the virus proteins would be regarded not as living but as autocatalysts of the type of pepsin and trypsin. The advantages and disadvantages of the two theories were discussed, and it was pointed out that, although the latter theory does not fit the experimental facts in that neither the existence of a precursor nor the production of a virus *de novo* has been demonstrated, additional work may eliminate these objections. The possibility that viruses may be elementary living organisms arising either by evolution or by retrograde evolution was discussed. It was pointed out that a molecule might have sufficient structure to endow it with unusual properties which might not interfere with its properties as a molecule but which might permit it to function as an organism when in the presence of a certain type of protoplasm.

SECTION OF PSYCHOLOGY

NOVEMBER 21, 1938

DOCTOR TRIGANT BURROW, The Lifwynn Foundation: *An Organismic Study of Behavior in the Individual and in the Community.*

There is a noteworthy feature that differentiates man's approach to his own behavior or motivation as contrasted with his approach to the behavior or motivation observable in other forms of life and to the phenomena of the external environment generally. Due to the adoption of controlled scientific methods in his relation to the outer environment, man has learned increasingly to maintain a clear-cut precision in respect to the data relating to the external management of things. In all our dealings with the reactions of animals and their motivation, in our study of plants and of minerals, our procedure is unfailingly objective and experimental. But with regard to processes that are internal to man—processes that motivate and direct the behavior of man's own organism as a species or phylum—man has failed to apply objective measures which would lead to similar progress. In our dealing with the behavior of man and his motivation our attitude is not experimental and objective but it is invariably prepossessive and traditional.

This direct relation of man to his outer circumstance or environment exists no less in the sphere of medicine as it applies to physical disorders objectively observable within man himself. Where inept reactions have occurred as a result of disharmony, disease or *dis*behavior in the outer environment, man has learned increasingly to apply immediate objective remedies to their adjustment or alleviation. But where there has occurred inept behavior, or disbehavior, in man's relation to man, where disorder has occurred in the relationships of men to one another subjectively, we have not only not applied the suitable objective remedies but instead the ineptness and disbehavior has become the accepted norm. Through the

force of sheer subjective habit, conflict has prevailed over man's processes, and disbehavior has displaced his basic behavior-reactions. The evidence of this widespread disharmony is seen in the prevalence of crime, war, industrial conflict, domestic infelicities, political, economic and religious discord. It would appear, then, that through some *faux pas* in his development man's behavior bears the stamp of a maladaptation or disbehavior in respect to himself that has superseded the primary reaction of his organism in relation to the immediate environment.

This evolutionary *faux pas* has to do with complications that have arisen in the relation of the symbolic segment to the organism as a whole. For the central configuration that presides over and preserves the consistency of the organism's symbolic field of perception on the one hand, and the central configuration that preserves the organism's world of internal feeling on the other have become entangled and confused in the pursuance of their appropriate objectives because of the too close approximation of these two major reaction-spheres to one another. In the work of my associates and myself our endeavor has been precisely to bring to book this confusion or overlapping between the organism's total feeling-reactions and the behavior based upon its external perceptual or symbolic code of reactions. Our interest in this challenge has been a preeminently practical one. For researches conducted over a good many years on the interreactions of individuals and groups afford unmistakable evidence that the very kernel of man's behavior-disorders, individual and social, are traceable to this confusion or conflict between spheres of the organism's motivation which require for their healthy functioning that they operate in definite coordination with each other.

Some eighteen years ago several of my associates, a number of my students and myself entered upon an experiment in group-analysis and in group-living which had as its purpose the blocking and frustration of such behavior-expressions as resulted from the overlapping and confusion of these two

spheres. It was not an easy job. Affective responses and behavior inconsistencies that are glossed over socially or mutually accepted by the participants came in for stern challenge. As a social group we undertook to challenge and defeat our accustomed level of interrelationships. The disparagement of these relationships had the effect of forcing the interest or preoccupation away from these socially current affectivities and of directing it instead toward the internal sensations that pertain to the organism's immediate behavior reaction. Instead of being occupied with the behavior of ourselves and of others we became aware of and interested in a physiological conflict in tensional patterns which accompanied the affecto-symbolic responses. Our aim, therefore, was henceforward directed toward the clear organization of these internally appreciable sensations.

This task was accomplished in part at least by intercepting the accustomed focus of attention upon the separate item or image as it occurs in ordinary thinking and, instead, allowing the sensations associated with the process of mentation to become perceptible in the internal neuromuscular reactions that accompany this activity. In this process of turning to these internal tensional patterns—a process called *cotention*—it was possible to discriminate between patterns related to man's conditioned part-functions and patterns related to man's basic adaptation. A physiological feature which was observed during periods of cotention consisted in a markedly slowed respiratory rhythm. In order to obtain more accurate measurements of these respiratory modifications, records were taken of the thoracic and abdominal respiratory curves by means of a wax paper kymograph devised by Volkmann and Gerbrands of the Harvard Psychological Laboratory. The diminution in frequency was from a normal average of 12 to 16 to an average of 3 to 6 respirations per minute during cotention. The alterations in the respiratory curve were typical and consistent in different experimental settings. It was possible to differen-

tiate these alterations sharply from slight modifications occurring under a variety of experimental situations.

It will be seen that, in phylobiological studies of human behavior undertaken by my associates and myself, we have attempted to view the problem of man's adjustment to man—neurosis, crime, social and economic disorders—wholly apart from the ideological prepossessions that prevail generally in respect to the obvious outer appearances of "mental" disorder. In having recourse to a frame of reference which regards disorders of behavior not as isolated disease-entities but as phyletic manifestations there would seem to be opened the possibility of a more consistent approach to the field of human conflict.